

# ATTENTION-AUTOMATIZATION: AN INVESTIGATION OF THE TRANSITIONAL NATURE OF MIND

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## ATTENTION-AUTOMATIZATION: AN INVESTIGATION OF THE TRANSITIONAL NATURE OF MIND<sup>1</sup>

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### I. EXPERIMENTAL AIMS

One occasionally hears the caustic comment that research is conducted in order to discover proof for preconceived theories rather than to formulate principles after the empirical evidence has been attained by strict laboratory methods. The researcher must be careful lest he be found among the dogmatists, who allow their biases to control their experiments. But it is evident that experimentation without a planned attack, without at least some warning of possible hypotheses to be developed, leads nowhere. The antecedent formulation of psychological theorems must be held as a scientific sin only when the worker is not frankly open to both favorable and unfavorable evidence.

The working hypotheses for our experiment were developed from a critical review of certain historical experiments on the nature of that supposed psychological state or process to which most texts refer as *attention*. More precisely stated, we are indebted to the work of J. J. B. Morgan<sup>2</sup> on distraction. Although terms, no matter how evasive, tend to commit one to conventional interpretations, we wonder to what extent Morgan's evasion of the word *attention*, or of any other term which signifies a theory,

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<sup>1</sup>From the Department of Psychology, University of Michigan.

<sup>2</sup>J. J. B. Morgan, The overcoming of distraction and other resistances, *Arch. Psychol.*, 5, 1916, (no. 35), 1-84.



may have prevented an arrangement of experimental data, well done from the standpoint of laboratory technique, from showing certain interesting principles of mental integration.

Although the term *attention* has been attached, in most of the historical researches, to a description of consciousness, there seems to be no great difficulty in showing attributes of behavior which are comparable to the structuralistic descriptions. Certainly it is not impossible to give Titchener's definition of the focal point of consciousness a strictly behavioristic formulation. One need not observe an animal for a very long period before seeing fairly definite indications of 'focal' and 'background' coordinations. Neither does it seem unreasonable to hold that there are definite indications of a sequence of acts, determined by previous environmental conditions, which evolve from each other in a selective order. Nevertheless, the dominance of a coordination and the corresponding suppression of acts which are not serving the same end as the coordination seem to be a purely relative matter, for there are types of coordination which interfere with each other to a minor extent only, and others which seem to be thoroughly antagonistic. An examination of the life-history of these coordinations appears to reveal something in the interference which is dependent upon a lack of repetitive use.

For example, one finds when learning to drive an automobile that it is practically impossible to perform the several acts which are required in a sudden emergency without a great amount of interference. Although each of the acts is consummated easily enough when carried out independently, simultaneity is impossible. When one has learned to drive the car well, however, one can actually perform the various acts of stopping with perfect synchronism while conversing. The mere fact of use seems to bring about a complete cessation of mutual interference and it may also give indications that the various components of the coordination are more efficient when carried out in the presence of each other.

The interference of one stimulus against another is so common an occurrence in everyday life as to suggest one of the most urgent problems concerning the 'process' nature of mind in the sense of a transition from new syntheses to habituated coordinations—from attention to automatization. We hear much about the structure of consciousness or the classification of behavior and very little about the transitional nature of mind; although many experiments have contained the principles in a mass of data better arranged to conceal than to demonstrate.

The history of work on distraction therefore prompts the following theorems.

(1) We may not frame any hypothesis in such a manner as to invalidate the known facts of nervous functions. The general subject of the acquisition of neural integrations may, when descriptive of mental actions or operations, be referred to under the

general phrase *psycho-dynamic gradients*, whether viewed from the standpoint of behavior, of neural function or of conscious phenomena.

(2) The repeated use of similar pattern-stimuli must reduce behavior to such a condition that specific interference is no longer present.

(3) Behavior selected under one set of conditions is a mental act different from the same behavior selected under other conditions. We argue for the classification of actions in terms of central determinants, not in terms of ends served.

(4) Automatization is essential in all acts of integration. It begins immediately and proceeds with greater and greater force until the coördination reaches a complete segregation from other systems of behavior. The speed of this tendency may be slow or rapid depending on the complexity of the act selected. *Attention* and *automatization* become the extremes of a transition accomplished through sheer repetition of the same stimulus-pattern.

(5) Resistance and acceptance reactions obey the same law. Avoidance of certain pattern-stimuli may become set off into automatization as well as the acceptance or selection of positive modes of behavior.

(6) There is no fundamental objection to considering attention as a process of association provided we differentiate between initial and subsequent stages. We may indicate, however, that the fact of dominance (clearness) and the facts of the psycho-dynamic sequence are two separate phases for consideration and should not both be labelled *attention*.

(7) A graphic representation called an 'attention' curve, made on the assumption that efficiency of behavior is a measure of attention, cannot remain level and constitute continuous attention. For example, training a subject in the use of problems to attain a level curve of efficiency is but to substitute automatization for attention. It defeats the purpose of the experiment. A slope in the curve of efficiency is in itself evidence that selection is operating and attention is present.

(8) The selection of a given type of behavior is probably never the result of any one condition but rather the result of composite forces of the entire environmental situation, and no measurement of attention is adequate unless all changes in the



environment are taken into consideration. The importance of this principle has grown more pronounced with the appearance of the *Gestalt* movement.

## II. HISTORICAL ANTECEDENTS

The nervous basis of reciprocal inhibition in the spinal cord was given its final verification by Sherrington.<sup>3</sup> A certain degree of application of the laws of spinal action to principles of cerebral action seems warranted on the basis of the facts of learning and memory. But we must not fail to note that every part of the spinal cord is connected with every other part. Sherrington writes, "Yet we must not lose sight of the physiological solidarity of the action of the group of elements that compose a 'reflex center' in its reflex activity. 'Immediate spinal induction' and the spatial spread of the refractory phase in the scratch-reflexes evoked from separate points show that, intra-spinally, the various component arcs of the type-reflex are connected to something like a unitary mechanism." The fact that events in one part of the nervous system modify the functioning of other parts ought to make the psychologist cautious in accepting any theory of association built on the concept of a single nervous tract. We cannot escape a mass of cross-relationships of one phase of behavior with another, the shifting of which should suggest many a fine problem in psycho-dynamic gradients.

The fact that researches upon attention have a tendency to become complicated with the process of automatization has been known since the earliest experiments by modern laboratory methods. It seems certain that Wundt was aware of this factor as a serious problem prior to 1880. Cattell<sup>4</sup> reported in 1886 that distraction stimuli were more effective on acts which were attended to for the first time. In spite of the fact that Cattell had published the results of his experiments involving the dangers of automatization, it does not seem to have occurred to anyone for a long time that the effect of distraction might not be a constant, but rather a persistent variable which is correlated with the newness of the integration. Therefore most investigators did not see that a temporal function was an important measure of the effect of distraction on attention. Almost all of the early experiments have assumed that, as long as the attended work was kept as variable as possible, all parts of a temporal sequence were equally well attended to. Often these early experiments have missed the possibility that a persistent common form running through the mental work could breed automatization just as the experienced bookkeeper learns to add new columns of old and familiar elements of organization; or just as the accomplished musician can read automatically a complicated new score because that score is cast in a form which makes use of old and familiar stimulus-patterns. It would seem that the failure to control the temporal aspect has been due to the traditional definition of attention as an attribute of dominance, without an adequate interest in the determinants which lead to that dominance.

<sup>3</sup>C. S. Sherrington, *The Integrative Action of the Nervous System*, 1923, Lect. 4.

<sup>4</sup>J. McK. Cattell, *Psychometrische Untersuchungen*, *Phil. Stud.*, 3, 1886, 329; The time taken up by mental operations, *Mind*, 11, 1886, 237-242.

In 1892 Swift<sup>5</sup> belittled the dangers of automatization, although acquainted with the work of Wundt's laboratory and Cattell's published work. Swift noted, however, that the effect of distraction diminished if the interference became "monotonous," failing to note anything in common between monotony and automatization. Apparently he seems to have missed the possibility that tendencies of resistance and acceptance are equally capable of automatization. He argued one out of existence but accepted the other. Swift concluded that the degree of attention was inverse to the amount of distraction from auditory and visual stimuli.

Münsterberg<sup>6</sup> tried the effect of introducing voluntary arithmetical work as a distraction to the process of making judgments on auditory intensities and visual distances. He had no check on the extent to which subjects resorted to attentional fluctuation between arithmetical problems and sensory discrimination, and no attempt was made to measure the extent to which the arithmetical operations had been reduced to automatized processes. Hamlin's<sup>7</sup> check on Münsterberg's experiment supported the former of the above criticisms. Her excellent review of research upon attention to that date gives a general indication of the previous failure to see attention as the termination of a gradient process, of selective conditions which operate differently with practice.

It was near the beginning of the present century that Titchener<sup>8</sup> laid the foundation for a definition of attention derived from his limitation of the field of psychology to the study of conscious processes. There is definite evidence in the writings of Titchener that he was interested in the functional descriptions of mind; but the prevailing problems in psychology at the beginning of the present century favored a static definition. Many of the experiments began on the problem of the relation of attention to sensory intensity and the possibility of the control of dominance by preceding mental conditions was often neglected. The procedure used in Münsterberg's experiment (previously cited) is an example of the point at issue. The prevalent acceptance of the terms *foreground* and *background* of attention without a more critical valuation of the evidence for these terms led to the repetition of Münsterberg's experiment by Hamlin. A careful examination of this experiment showed that the problem did not logically concern the relation of background and foreground clearness at all, but was rather a problem of the selective factors for dominance. Hamlin says, "We notice first the evidence upon the nature and effects of the 'distraction.' The subjects usually found that it acted as a spur rather than a check on attention. Their attention was wholly absorbed in the addition for a part of the interval, but as a rule the distractions were not continuous, and there came free instants in which the stimulus flashed into

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<sup>5</sup>E. J. Swift, Disturbance of the attention during simple mental processes, this JOURNAL, 5, 1892, 1-19.

<sup>6</sup>Hugo Münsterberg, The intensifying effect of attention, this JOURNAL, 1, 1894, 39-44.

<sup>7</sup>A. J. Hamlin, Attention and distraction, this JOURNAL, 8, 1896, 3-66.

<sup>8</sup>E. B. Titchener, Postulates of a structural psychology, *Philos. Rev.*, 7, 1898, 449-465.



consciousness with great clearness and distinctness." How many times have the reports on background clearness been due to these "free instants," *i.e.* momentary fluctuations!

Certainly there is little evidence here for a description of background in the usual sense. We must note that the incentives to attend, as a condition of the selective process, are altered the moment the distraction is used. If our criterion holds true that behavior under one set of selective conditions is a different mental act from the same behavior under other selective conditions, then it seems that we have an adequate explanation of Münsterberg's and Hamlin's results, not in terms of the effect of attention on sensory intensity, but rather in terms of the differences in effectiveness of the conditions of attention.

Most of these experimenters averaged together all reactions, whether they came in the beginning, the middle, or at the end of the experiment, failing to take into account the possibility of a change in the nature of the mental process as the individual did the same kind of work over and over again. Have we here the results of automatized acts averaged in with the results of volitional or attended acts without differentiation? Some weak attempts were made to control automatization. The initial 'practice curve' has long been recognized. But even in this control there has been the persistent uncritical assumption that after a subject has passed through the period of practice he is still exhibiting acts of attention. It seems astonishing that so many experimenters have given no indications of the possibility that the practice curve itself may be the only part of the behavior that is really to be called attention, a persistent gradient always found in the initial stages of newly integrated behavior.

Some of the early experiments are interesting examples of the above lack of control. In 1896 Birch,<sup>9</sup> working in the Cornell laboratory, used odors as a method of distracting attention given to the discrimination of pairs of sounds. The experiment was partially lacking in the control of the temporal factor, but there are one or two outstanding results which appear to be important in the development of our theory of attention. Birch deplored the fact that she had not given a great amount of time in the training of her subjects for the mental work which was to constitute a measure of attention. She found that the curve of attention constantly seemed to fall during the intervals of no distraction. As the subjects were given more and more periods of work there seemed to be a tendency for the number of correct judgments to increase under the effects of distraction. She was afraid that this was due to lack of previous training; but there is a possibility that this would always occur under continued distraction and is but an indication of automatized avoidance of the disturbing stimulus.

Darlington and Talbot,<sup>10</sup> in the same laboratory, attempted to show the effect of short musical phrases as a distraction. Their general conclusion was that discrimination was better during distraction than without distraction.

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<sup>9</sup>L. G. Birch, A study of certain methods of distracting the attention, this JOURNAL, 9, 1897, 45-55.

<sup>10</sup>L. Darlington and E. B. Talbot, A study of certain methods of distracting the attention, this JOURNAL, 9, 1898, 332-345.



But again they gave no curves to show how rapidly this capacity to work better under distraction developed, whether it was abrupt and constant, or gradual and accumulative. All we can conclude from this experiment is that some conditions for selection are better than others. Fear that one is going to be bothered is apparently an indication of a special selective condition and may be accompanied by a pronounced difference in efficiency of behavior. The neglect of the temporal attribute seems to be a persistent tendency among investigators holding a theory of attention as a state. Titchener's<sup>11</sup> formulation of attention in terms of attributive clearness was followed by experiments carrying his basic assumptions. The year after the publication of Titchener's book Geissler<sup>12</sup> brought forward an experiment on the relation of attributive clearness to efficiency of behavior, based on visual processes. Four years later the work was carried forward in a similar way by Dallenbach<sup>13</sup> who attempted to correlate attributive clearness of auditory states with efficiency of behavior. He gave a correlation (product-moment method) of .85 as the lowest result when comparing efficiency of work performed with verbal reports on clearness.

Dallenbach says, "Previous experimenters have found that distractors very soon lose their power of compelling the attention; the observers become habituated." Here we have a direct allusion to the fact that resistance to a distraction can become automatized. Here also we seem to see, for the first time, a real attempt to control the problem, for Dallenbach tried to keep the distractions constantly varied in order that 'habituation' might be kept at a minimum. Whether he succeeded or not could only have been told by some sort of a temporal curve, showing the changes in efficiency as the various distractions continued. In all probability, even a variable distraction can ultimately be resisted. But it is a very significant fact that Dallenbach reversed the opinions of previous investigators in the statement that the distractions impaired efficiency instead of increasing it. It would seem that this conclusion would not have been possible had he not been very careful in controlling the fact of 'habituation.' We need not quibble concerning Dallenbach's method of securing evidence of attention. Morgan's substitution of a direct measurement of overt behavior for the verbal report or 'introspective report' has nothing to do with the principle. Since the correlation between the two types of behavior is high we may regard either as an adequate measurement of attention. The fact is, we seem to have here evidence that Dallenbach measured the *initial* effects of distraction, while previous experimenters had been measuring *cumulative* effects and securing, therefore, opposite and contrasting results.

It is evident that so far there were no attempts to show, by means of a smooth curve, how distractions can affect efficiency of behavior as time passes. Bits of incidental evidence occasionally were given, but not with the motive

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<sup>11</sup>E. B. Titchener, *Lectures on the Elementary Psychology of Feeling and Attention*, 1908, Lect. i, v, and vii.

<sup>12</sup>L. R. Geissler, The measurement of attention, this JOURNAL, 20, 1909, 473-529.

<sup>13</sup>K. M. Dallenbach, The measurement of attention, this JOURNAL, 24, 1913, 465-507. Cf. also The measurement of attention in the field of cutaneous sensation, *ibid.*, 27, 1916, 443-460.

of showing the process nature of the resistance to distraction. Indications of the principle may be noted in some of the studies on the curve of efficiency for the acquisition of skill. Batson<sup>14</sup> noted that a distraction was more effective in the beginning than toward the end of the acquisition of a skilled performance. He held that the effect of a distraction is never totally lost, a statement that seems to indicate that we are dealing only with an approach to limits—a psychological application of the notion of relativity. Some of the experiments on reaction-time under the influence of distraction remind one of the days of the older Wundtian laboratory and the warning of Cattell on the importance of automatization. The contribution of Evans<sup>15</sup> on the relation of distraction to reaction-time is an example. Evans used light, sound, and touch distractions in simple reaction-time experiments and obtained temporal curves which indicated a definite gradient. The repetition of a simple reaction implies a fairly rapid automatization rate and would not be regarded by all as a good example of the effect of distraction on attention. As a matter of fact, we see no reason why Evans' curves may not be regarded as a measure of the attention-automatization gradient.

Dissatisfied with Dallenbach's measurement of attention by introducing verbal reports on attributive clearness, Morgan<sup>16</sup> decided to carry forward a distraction experiment in which all computations should be based on immediate and direct behavioristic measurements. Because he apparently felt that there was no safe criterion as to what constituted attention in terms of behavior, Morgan seems to have decided definitely to avoid the word *attention*, allowing his readers to make whatever inferences were desired as to the principles of mental control behind behavior. His experiment is generally classified as one on attention, from its apparent resemblance to traditional experiments.

Morgan found that the average reaction-time for the solution of difficult problems is less under noise than under quiet conditions. He also found that the first problems in a series under distraction require a longer time for solution than later ones. The increase in speed for the later problems more than compensated for the slowness in working the initial problems of the series. His method of averaging together ten or more problems concealed the possibility of a smooth curve of distraction. The fact that he did not average the entire period together was the saving point in his technique of computation; it left something for the process nature of adaptation to distraction. He measured the concomitant motor effects by means of key pressure and found that the increase of efficiency was accompanied by a dynamogenetic effect of distraction. Although Morgan's statement that distraction increased efficiency as measured by speed, we must remember that this statement is the result of averaging together several problems in the same distraction period and the statement means just what the older statements meant, *i.e.* the cumulative effects of distraction, or automatized resistance to distraction accompanied by increased incentives to work, not the initial effects of distraction on attention as measured by

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<sup>14</sup>W. H. Batson, Acquisition of skill, *Psychol. Monog.*, 21, 1916, (no. 91), 86.

<sup>15</sup>J. E. Evans, The effect of distraction on reaction-time, *Arch. Psychol.*, 37, 1916.

<sup>16</sup>Morgan, *op. cit.*



Dallenbach. The fact that Morgan did take into account the difference between initial and cumulative effects of distraction and mentioned the difference is the saving point in his work which suggests the possibility of a point of view leading to a definition of attention and automatization as extremes of a gradient process. Morgan used noises for distraction, which were unique in that they were unusually severe. The same type of noise was used throughout one sitting.

The precision of Morgan's experiment places it in a position of importance in solving the problems relating to the nature of attention. Direct experiments on the problem have not been attempted in the European laboratories; but there are some researches which seem to contribute interesting possibilities of explanation. Claparède,<sup>17</sup> for instance, had begun an experiment on fatigue which resolved itself later into an experiment on attention. When a subject was given the task of contracting both hands simultaneously on a pair of ergographs until paralysis resulted, the cessation of *trying* to contract one hand was accompanied by a resumption of actual movement of the other hand although no rest-pause had been given. Now the sudden resumption of movement by a member that was previously paralyzed with fatigue seems to offer something important in the nature of the central controls of volition. Claparède suggested an explanation in terms of the drainage of energy from central reservoirs which had been divided while innervating two hands, but which are later allowed to throw their entire force into one hand. In terms of behavior, we know that there was present an interference of efficiency when two hands were used, and it would seem that the postulation of reservoirs carries us beyond scientific necessity.

An extension of Claparède's work was accomplished by one of his pupils, Rimathé,<sup>18</sup> using a similar ergograph, but so arranged that homolateral pairs of muscles could be used. He found that the same principles held for muscles on the same side of the body, an indication that the phenomenon could not be correlated with the paired functions of the cerebral hemispheres.

The fact seems to be, however, that we have here what is essentially a distraction experiment. Verbal reports on the part of subjects in such fatigue experiments indicate that the process of producing fatigue-paralysis is accompanied by exceedingly uncomfortable feeling from the members being used and that there were changes in incentives when the fatigue pains were removed from one of the hands—enough to account for the increase of efficiency. While Morgan did not carry his experiments far enough to produce a great amount of sensible fatigue, we may indicate here that in all probability we must be on our guard against the inhibiting effects of proprioceptive as well as exteroceptive stimuli, and regard them as possibilities of distraction.

### III. APPARATUS AND PROCEDURE

With the foregoing criticisms in mind, we wished to show the changes in efficiency for each successive problem carried on with-

<sup>17</sup>E. Claparède, *L'ergographie bilatérale*, *Arch. des sci. phys. et nat.*, 44, 1917, 71.

<sup>18</sup>F. Rimathé, *Contribution à l'étude de l'ergographie bilatérale et simultanée*, *Arch. de psychol.*, 19, 1924, 128-162.

out disrupting the temporal order, both with and without distraction. At the same time we wished to make the period of work short enough to reduce the chance of extreme automatization of the problems. It will be seen that, instead of eliminating the practice curve, we have deliberately kept our observations within its scope. Efficiency of behavior will be measured in terms of immediate and direct performance, with enough verbal report allowed to throw more light on the causes of irregularities. In order that the cumulative effect of distraction might be taken into account we decided to average together all first problems apart from the average of all second problems of the series, and these in turn apart from all third problems, and so on. Our graphs will represent the function of reaction-time plotted against position in the series.

In spite of the fact that Morgan's problems were much better than those used in many other experiments, in that they were much more difficult and therefore less rapidly automatized, we believed that it would be possible to frame problems which were equally difficult and which at the same time placed a penalty on any fluctuations of attention from the work at hand. Any deviation from the following problems will endanger forgetting the sum which *S* is carrying and produce a corresponding long reaction brought about by the necessity of going back to the beginning of a problem, or there may be a false answer.

Ordinarily arithmetical operations may be automatized as generalized pattern-situations; so if they are to be used they must be placed in unusual combinations or environments. We framed 60 problems involving the selection of single digit numbers from a row of mixed letters and numerals, *S* being told to add these numbers as he discovered them and to write their sum on a strip of paper. The following is an example of one of the problems:

Gd7Fc8NfEaW9MBcOPT5FzA4NvCXz6MkhgP9xtb.

The numerals were scattered throughout the series of symbols in no predictable order either as to spacing or kind. The symbols were secured by chance as was also the spacing of the various digits.

The accumulation of muscular tonus during the distraction was an important point in Morgan's work and was made an index to the intensity of the mental process. We had no objection to Morgan's method of denoting the presence of the factor and had no doubts concerning his statements of the results. Because our *Ss* wrote their answers on paper instead of pressing keys, we used writing pressure instead of key pressure, indicating the amount by means of a tambour recorder on a strip of kymographic paper. The same pencil was used throughout all runs of the experiment in order that there might not be variations of behavior due to differences in hardness of the writing point.



Morgan's distractions, as we have indicated, were more than usually severe and we agreed on the whole with his selection. We had but one difficulty in selecting the noise to be used, the matter of the difference between monotonous or constant noises and variable noises. If we used the evidence based on a comparison of Morgan's and Dallenbach's results, the former would produce rapid and the latter slower automatization. We did not think it necessary to use the great variety of noises introduced by Morgan, since many of them must have given identical results.

Our choice finally fell on two methods of creating a noise distraction, a Klaxon automobile horn placed about two feet from *S*'s ear, and a phonograph operated with a loud needle, the amplifying horn being adjusted as closely as possible to *S*'s head. A monologue in a male voice delivered its supposedly humorous story into the ear of *S* while he was trying to solve the problems outlined above. In the preliminary experiment the phonograph was used entirely and in the later experiments we used the automobile horn.

The room in which *S* sat was illuminated entirely by artificial light, the walls being painted black and free from all light-distracting objects. The experiment was conducted in a section of the building which was relatively free from extraneous and uncontrolled noises. A 60-w. lamp overhead gave a small amount of illumination all the time, while another 60-w. lamp on the table at *S*'s side was so screened that it threw a bright light on the card of problems held in front by a bracket. The table lamp was so connected that *E* could, without entering the room, throw it on as a signal to begin work and turn it off as a signal to stop. *S* was always alone in the room while doing the problems; the kymograph, recording instruments, motor and control switches were all in another room, sound-insulated from him.

The apparatus on *S*'s side of the partition consisted of the following items. An endless belt, for exposing the code numbers indicating which problem was to be solved, was arranged in such a manner that the pushing of a lever turned a new code-stimulus into view and at the same time drew a fresh space on the strip of white paper used for recording the answers. The strip of paper on which the answers were written was drawn on a spindle so as to run over a large tambour on which very stiff rubber had been stretched and then covered with a wooden plaque. The tambour thus constituted an indicator of writing pressure. The amount of deflection of the tambour was imperceptible to the *S*s, only four of whom knew that the writing-pressure was being recorded. A tube extended through the wall from the writing-pressure board to the tambour-recorder on the kymograph. The tension of the writing-pressure board was calibrated by placing weights on the paper and noting the amount of deflection of the recording needle.

A Lombard pneumograph was strapped by means of a broad leather band to the upper portion of the thorax and was used for the same purpose as that used by Morgan, *i.e.* to secure indications of articulation. Since blood circulation was also being measured the respiratory changes constituted one of the checks on vaso-motor activity. The left fore-finger and second finger were pressed firmly to a rubber bulb and fastened to it by means of adhesive tape. A tube led from the bulb to the piston-recorder, the usual arrangement for the

Lombard-Pillsbury plethysmograph. The arm of *S* was now suspended in a sling hung from the ceiling in order to allow for body-movements without interfering with the operation of the plethysmograph. It was hoped that this portion of the apparatus might produce evidence in addition to Morgan's statements on articulation and key-pressure.

The kymograph used was developed in our laboratory.<sup>19</sup> It employed high tension discharge of electric sparks as a means of making a graphic record on white paper. The graphic record consisted of the following lines. (1) The perforated line of blood-volume taken with the piston-recorder. (2) The time-line (in sec.) taken with period perforations controlled by connecting the spark discharge in series with a mercury-contact seconds-clock. Some of the records were taken with the kymograph being driven by a synchronous motor operating on 60-cycles current from the power-house. Checks were made frequently for variations of speed, but as the variability of the motor was less than 1% of the variability of the data, there seemed to be no reason for rejecting the method of measuring time in terms of relative linear distances on the kymograph record. (3) A perforated line from the needle of a tambour giving a record of breathing changes. (4) A perforated line controlled by *E*'s switch indicating the exact moments the distraction was turned on and off. 5) An ink-needle tambour giving a record of the amount of writing-pressure exerted by *S*, and also a means of ascertaining the writing-time which was not counted with the total reaction-time.

At the beginning of the first sitting, *E* told *S* the nature of the problems and worked one out, in order that the procedure might be perfectly clear. The noise was turned on for a moment in order that *S* might become acquainted with the nature of the distraction. This appeared necessary, especially with the automobile horn, because of the tendency of the *Ss* to jump from their chairs if the distraction were turned on without previous warning. *S* was told then to start working just as soon as the lamp on the table lighted, to work as rapidly as he could, to remember that both speed and accuracy records were being taken, and to permit no interference from anything until the light on the table was extinguished.

*S* worked upon not more than 8 of the problems, when the distraction was turned on at exactly the beginning of a problem. Thus the preliminary quiet period was, on an average, about a minute-and-a-quarter long. The distraction remained on for exactly 6 problems, then it was abruptly discontinued. Then there followed 6 or more problems worked in absolute quiet. The 6 problems before the distraction, the 6 during the distraction and the 6 after the distraction were measured and these measurements given a place in frequency curves, a curve being made for each position in the series. Although there were often as many as 8 problems before the distraction, only the 6 preceding the distraction were tabulated. We varied the number of problems in the first and third periods in order to prevent anticipation of the end of the period by the *Ss*. In the first period this prevented fear of the distraction affecting the final problem, and in the third prevented the possibility of the 'end spurt.' It

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<sup>19</sup>Adelbert Ford, Recording apparatus: the electrokymograph, *J. Exper. Psychol.*, 7, 1924, 157-165.



is true that the working period in our experiment was much shorter than in Morgan's. Some of the *Ss* complained that they had scarcely got 'warmed up' to the work when they were forced to stop. But it must be remembered that we were working under the assumption of the possibility that excessive training removes attention, that the best place for measurement of attention is in the initial stages of the practice curve.

After each sitting *S* was asked to describe what happened while he worked. Since the results of the experiment were to depend heavily on the efficiency of performance, this 'introspection' was introduced merely to secure evidence concerning any unpredictable conditions. After they had finished their voluntary description they were asked a few direct questions: "Did you have any emotion?" "Did you lose your place while working?" "What happened when the noise stopped?" "Have you ever had experience in an occupation which required a great amount of arithmetical addition?" "What effect do you think the noise had on your speed and efficiency?" "Did you have any difficulty with the manipulation of the apparatus?" They were also asked to make statements concerning health, age, state of rest or fatigue, and mood.

During the first run there were probably other distractive conditions than the noise itself. The *Ss* were not accustomed to the conditions of the psychological laboratory. Many were more or less nervous upon entering and being seated in an eerie, black room, knowing that "something in their mind was being measured." The experience of being strapped into various instruments was another source of novel stimulus which was certain to bring about changed attitudes toward the work at hand. But there was strong evidence that these uncontrolled environmental and attitudinal effects rapidly disappeared. Several of the *Ss*, forgetting that they had a bulb in their fingers and a pneumograph on their chests, walked off as soon as the experiment ended, tearing the instruments from their respective tubes. The nervousness from the fear of being "mentally measured" likewise disappeared after the first run and they reported taking the experiment as a matter of course. All but three of the *Ss* were undergraduate students in elementary psychology. We have since regretted that we did not give every *S*, before the principal experiment, a period of special work to produce environmental orientation. From the point of view we have taken here, this would mean "automatization to uncontrolled distractors." In all probability our *Ss* did not get accustomed to the laboratory environment until the second sitting of the experiment.

It may be said that the introduction of an automobile horn as a distraction was likely to produce emotional effects which constituted an unmeasured variable. This criticism cannot be answered in any form of distraction experiment. There are always emotional and feeling effects and they cannot be totally eliminated. It may even be possible that the situation which brings about new integrations necessitating strong dominance, *i.e.* attention, involves the very conditions which make emotions unavoidable. We are not at all certain that in all situations where one pattern opposes another the emotion is not the working principle of the distracting effect.

## IV. THE PRELIMINARY EXPERIMENT

The first runs in the experiment were started in the fall of 1921 and were made largely with a view toward perfecting the apparatus. Apparently the phonograph did not give nearly as much of an emotional shock as did the automobile horn. While we had thought that the phonograph might constitute a variable distraction, and so prevent automatization, we found that most of the *Ss* reported that they heard nothing from the instrument that was more than vaguely meaningful, there being only an occasional awareness, confused and fragmentary, of a male voice talking in strident tones. This seemed to have reduced the effectiveness of the phonograph to that of a monotonous distractor, much inferior to the horn. In the later experiments we abandoned the phonograph.

TABLE I  
PRELIMINARY EXPERIMENT

Measurements of time (sec.) required for the solution of problems. An experimental period consisted of 18 consecutive problems: 6 under quiet conditions, 6 under distraction (phonograph), and 6 under quiet conditions again. Based on frequency curves of 68 experimental periods

| Mode   | Median | Mean   | M.D.  | M. - M.D. | M. + M.D. | Errors |
|--------|--------|--------|-------|-----------|-----------|--------|
| 11.847 | 14.733 | 16.176 | 4.794 | 11.382    | 20.970    | 11     |
| 12.176 | 15.000 | 16.412 | 4.457 | 11.955    | 20.869    | 6      |
| 13.494 | 15.400 | 16.353 | 4.467 | 11.886    | 20.820    | 7      |
| 13.851 | 15.833 | 16.824 | 4.650 | 12.174    | 21.474    | 15     |
| 13.193 | 15.429 | 16.412 | 4.747 | 11.665    | 21.159    | 14     |
| 13.294 | 16.000 | 17.353 | 4.492 | 12.861    | 21.845    | 11     |
| 16.273 | 18.091 | 19.000 | 5.149 | 13.851    | 24.149    | 11     |
| 14.352 | 16.000 | 16.824 | 4.269 | 12.555    | 21.093    | 12     |
| 19.811 | 18.077 | 17.210 | 3.641 | 13.569    | 20.851    | 14     |
| 14.381 | 16.833 | 18.059 | 4.704 | 13.355    | 22.763    | 10     |
| 15.811 | 17.545 | 18.412 | 4.808 | 13.604    | 23.220    | 6      |
| 13.823 | 16.333 | 17.588 | 4.375 | 13.213    | 21.963    | 16     |
| 15.624 | 16.600 | 17.088 | 3.845 | 13.243    | 20.933    | 16     |
| 15.014 | 15.750 | 16.118 | 3.659 | 12.459    | 19.777    | 12     |
| 13.880 | 16.058 | 17.147 | 3.784 | 13.363    | 20.931    | 7      |
| 14.732 | 16.636 | 17.588 | 3.975 | 13.613    | 21.563    | 15     |
| 15.051 | 16.429 | 17.118 | 4.014 | 13.104    | 21.132    | 12     |
| 12.737 | 15.363 | 16.676 | 3.973 | 12.703    | 20.649    | 10     |

The figures in Table I give the results of 68 experimental runs, an average of four from each person, in terms of the computed mode, median, mean, and mean deviation, and the number of errors committed out of 68 problems, the phonograph being used as a distraction. We used the mean as a measurement of the central tendency throughout all work. The median and com-





FIG. 1

puted mode were given for reasons to be considered later. Diagram A (Fig. 1) shows the facts of the table in graphic form wherein the vertical coördinate represents reaction-time and the horizontal coördinate represents the series of problems worked. The levelness of the reaction-time measurements for the first 6 problems in all the graphs rather gives one the notion that the slope of the curve of practice is very gradual. The values in all tables for every problem in the series are based on averages of 38 runs or more. The headings of the tables give the number of measurements averaged together.

The following points are to be noted in the curve of means for Table I, based on 68 runs, with the phonograph as a distraction:

- (1) The practice effect is not present.
- (2) The initial problem in the distraction period displays a conspicuously greater reaction-time than any other in the series. The entire 6 problems of the distraction period are longer than those of the quiet periods.
- (3) There is no evidence of an initial long reaction-time for the last quiet period.
- (4) There is a very poor correlation between writing-pressure and the reaction-time.

## V. METHODS OF COMPUTATION

We have given the mean deviation in our tables for the purpose of indicating any changes in the variability of efficiency under various conditions. The fine lines on the diagrams show the limits of mean deviation on each side of the mean. The causes of deviation are important for consideration, although to some extent problematical. We should probably have to consider differences in *ability* in this type of arithmetical work, differences in the *difficulty* of the problems being used, changes in the *attitude* of the subjects from problem to problem, *fatigue* effects, *minor distractions* from discomfort and other organic conditions, as well as other factors impossible to identify.

Practically all of the frequency curves secured for every problem in the series show evidence of skewness. The measurement for the skewness of a curve was suggested by Pearson, and we have given enough of the curve measurements to enable computation of this factor. Pearson had noted that in most skewed curves the distance of the mode from the mean was three times that of the distance of the median from the mean. This gave him a means of computing the mode, given the mean and the median, with but small error from the position



of the observational mode. Our use of the computed mode in our tables is presented solely as a means of denoting the relative amount of skewness. Pearson's suggestion for a formula for skewness which would not be expressed in original units was

$$\text{Skewness} = \frac{\text{Mean} - 3(\text{Mean} - \text{Median})}{\text{Standard Deviation}},$$

but there is no essential reason for abandoning the original units as long as we make comparisons in homogeneous data. Therefore it seems that the difference between the mean and the median may be taken in our tables as an indication of the skewness.

The meaning of skewness must affect our interpretation of results. Whenever the mean shows a definite change, either higher or lower, we must know whether the change was caused by a few extreme and exceptional cases or was brought about by a tendency for a small amount of change in all cases. If the distraction, for instance, should cause an *S* to forget his accumulated sum, he might occasionally have to go back and pick up his work from the beginning of that particular problem. This might not occur often; but such a condition would have a tendency to add cases on the end of the curve already having the broader range and increasing the skewness more than ever. On the other hand, if the distraction has the effect of destroying automatization and *S* is merely displaying a slower associative tendency, we might expect all the reactions to be longer by a fairly constant increment. It is evident that the increase in skewness would be accompanied by a small increase in the mean deviation. If the distraction had the greater effect on the shorter reactions, *i.e.* the ones which were successfully completed without the subject losing his place, then the mode would be thrown farther away from the negative end of the distribution and the amount of skewness would be seen to decrease.

The amount of skewness in Table I decreases the moment *S* begins to work under distraction. The mode is thrown nearer the positive end of the distribution, an indication that the effect of the distraction is probably general on all reactions and not just a pronounced effect of a few reactions which are the result of *S* losing his sum and having to go back to the beginning of the problem.

Although our *Ss* showed a greater tendency to commit errors than did Morgan's, there seems to be no indication of a correlation between number of errors and the presence of distraction. It is true that there are more errors in the first half of the distraction-period than in the last half; but in view of the great variability of errors from time to time it seems that we cannot be sure that this is not coincidence.

In the experiments where we used the phonograph as a distractor there seemed to be very little evidence of any correlation between the writing-pressure used by the *Ss* and the length of

time for working out the problems, and no observable transition in writing-pressure at the time the distraction appeared. We are inclined to believe that this failure to get results was due to the weakness of the distraction used.

Reports by the *Ss* seemed to give some clues as to mental attitude.

Most *Ss* reported annoyance at the beginning of the noise-period and relief at the beginning of the following quiet-period. Nearly all were able to state that they were retarded at the beginning of the noise-period, but only 4 that they recovered speed as the noise-period progressed. None of the *Ss* were able to tell the story given in the monologue even after the fourth repetition of the same record. One *S*, a graduate student in psychology, reported after the first run that she had not heard the record at all and did not know that the phonograph had been turned on. She apparently received no effect at all from the distraction even by measurement of the reaction-times. No other *S* ever reported complete obliteration of the distraction. Five of the 17 *Ss* in this first group reported that they customarily studied with a phonograph playing in their rooms, a temporary fad among students in the University at that time. The composite curve of these 5 *Ss* showed such great variability that we cannot state the influence of the previous training.

## VI. LATER EXPERIMENTS

Whereas in the preliminary experiment with the phonographic distraction we had used 16 *Ss*, in the fall of 1922 we arranged to use 41, 38 undergraduates and 3 graduates in psychology. We corrected the apparatus in such a manner as to simplify *S's* work in changing problem-cues, and to have a finer calibration to the writing-pressure board. The same problem card was used as before. We moved the spark commutator into a distant room to prevent the least possible noise, and used a quiet synchronous motor for moving the kymographic mechanism, measuring time by relative distances on the kymograph-paper. We substituted the automobile horn for the phonograph.

Table II gives the results of 183 sittings from 41 *Ss*. Diagram B (Fig. 1) shows the results in graphic form. Table VI shows the measurements of writing pressure in these same sittings. Table III is a composite result of first sittings for 38 *Ss*, the first run only. Diagram C (Fig. 1) gives the graphic illustration. Table VII gives the writing-pressure measurements for Table III. Table IV and Diagram D (Fig. 1) give the results of the second consecutive sitting for the same 38 *Ss* and Table VIII gives the corresponding



TABLE II  
TIME MEASUREMENTS  
From frequency curves of 183 experimental periods

| Mode   | Median | Mean   | M.D.  | M. - M.D. | M. + M.D. | Errors |
|--------|--------|--------|-------|-----------|-----------|--------|
| 15.621 | 19.167 | 20.940 | 6.259 | 14.681    | 27.199    | 47     |
| 13.463 | 18.579 | 21.137 | 6.425 | 14.712    | 27.562    | 41     |
| 16.751 | 19.857 | 21.410 | 6.036 | 15.374    | 27.446    | 34     |
| 13.359 | 19.273 | 22.230 | 6.179 | 16.051    | 28.409    | 34     |
| 18.061 | 20.833 | 22.219 | 6.150 | 16.069    | 28.369    | 40     |
| 17.253 | 19.769 | 21.027 | 6.123 | 14.904    | 27.150    | 34     |
|        |        |        |       |           |           |        |
| 18.947 | 21.667 | 23.027 | 6.250 | 16.777    | 30.277    | 34     |
| 15.741 | 18.923 | 20.514 | 5.899 | 14.615    | 26.413    | 52     |
| 16.659 | 19.571 | 21.027 | 5.613 | 15.414    | 26.640    | 45     |
| 17.152 | 19.320 | 20.404 | 5.669 | 14.735    | 26.073    | 30     |
| 15.983 | 19.091 | 20.645 | 6.235 | 14.410    | 26.880    | 31     |
| 13.067 | 17.667 | 19.967 | 5.737 | 14.230    | 25.704    | 45     |
|        |        |        |       |           |           |        |
| 18.402 | 20.684 | 21.825 | 6.211 | 15.614    | 28.036    | 43     |
| 16.339 | 19.421 | 20.962 | 6.016 | 14.946    | 26.978    | 40     |
| 15.712 | 18.840 | 20.404 | 5.704 | 14.700    | 26.108    | 41     |
| 15.249 | 18.933 | 20.775 | 5.856 | 14.919    | 26.631    | 42     |
| 18.249 | 19.555 | 20.208 | 5.788 | 14.420    | 25.996    | 35     |
| 15.103 | 18.739 | 20.557 | 6.291 | 14.266    | 26.848    | 32     |

TABLE III  
TIME MEASUREMENTS  
The work of 38 Ss without previous training

| Mode   | Median | Mean   | M.D.  | M. - M.D. | M. + M.D. | Errors |
|--------|--------|--------|-------|-----------|-----------|--------|
| 27.181 | 26.429 | 26.053 | 7.216 | 18.837    | 33.269    | 8      |
| 22.642 | 24.600 | 25.579 | 6.820 | 18.759    | 32.399    | 4      |
| 20.315 | 25.333 | 27.842 | 8.141 | 19.701    | 35.983    | 5      |
| 23.842 | 25.000 | 25.579 | 5.684 | 19.895    | 31.263    | 11     |
| 26.078 | 27.500 | 28.211 | 7.612 | 20.599    | 35.823    | 13     |
| 23.484 | 24.600 | 25.158 | 5.745 | 19.413    | 30.903    | 6      |
|        |        |        |       |           |           |        |
| 22.998 | 25.666 | 27.000 | 6.684 | 20.316    | 33.684    | 9      |
| 20.704 | 23.500 | 24.895 | 5.983 | 18.912    | 30.878    | 11     |
| 22.706 | 24.200 | 24.947 | 6.310 | 18.637    | 31.257    | 11     |
| 21.790 | 24.000 | 25.105 | 6.111 | 18.994    | 31.216    | 8      |
| 27.000 | 27.000 | 27.000 | 6.263 | 20.737    | 33.263    | 7      |
| 25.210 | 25.000 | 24.895 | 5.947 | 18.948    | 30.842    | 12     |
|        |        |        |       |           |           |        |
| 20.898 | 25.000 | 27.052 | 8.291 | 18.761    | 35.343    | 11     |
| 25.686 | 26.000 | 26.157 | 6.174 | 19.983    | 32.331    | 13     |
| 25.422 | 25.000 | 24.789 | 7.105 | 17.684    | 31.894    | 11     |
| 23.484 | 24.600 | 25.158 | 5.640 | 19.518    | 30.798    | 12     |
| 19.314 | 26.666 | 25.842 | 7.291 | 18.551    | 33.133    | 9      |
| 21.106 | 23.000 | 23.947 | 7.896 | 16.051    | 31.843    | 8      |

TABLE IV  
TIME MEASUREMENTS

The work of 38 Ss one week after the first run tabulated in Table III

| Mode   | Median | Mean   | M.D.  | M. - M.D. | M. + M.D. | Errors |
|--------|--------|--------|-------|-----------|-----------|--------|
| 18.015 | 20.111 | 21.159 | 4.455 | 16.704    | 25.614    | 10     |
| 18.794 | 19.666 | 20.102 | 5.064 | 15.038    | 25.166    | 12     |
| 22.629 | 21.859 | 21.474 | 4.728 | 16.746    | 26.202    | 4      |
| 21.106 | 21.000 | 20.947 | 4.947 | 16.000    | 25.894    | 4      |
| 16.986 | 20.750 | 22.632 | 5.152 | 17.480    | 27.784    | 6      |
| 19.868 | 21.500 | 22.316 | 4.997 | 17.319    | 27.313    | 8      |
| 17.842 | 23.000 | 25.579 | 6.324 | 19.255    | 31.903    | 4      |
| 16.394 | 19.500 | 21.053 | 5.535 | 15.518    | 26.588    | 12     |
| 19.776 | 21.750 | 22.737 | 5.773 | 16.964    | 28.510    | 14     |
| 14.800 | 19.600 | 22.000 | 5.895 | 16.105    | 27.895    | 1      |
| 18.893 | 20.333 | 21.053 | 5.058 | 15.995    | 26.111    | 5      |
| 18.761 | 20.429 | 21.263 | 5.186 | 16.077    | 26.449    | 9      |
| 19.974 | 21.500 | 22.263 | 4.936 | 17.327    | 27.199    | 9      |
| 20.998 | 21.666 | 22.000 | 5.263 | 16.737    | 27.263    | 7      |
| 17.946 | 19.666 | 20.526 | 4.792 | 15.734    | 25.318    | 10     |
| 18.474 | 21.000 | 22.263 | 5.883 | 16.380    | 28.146    | 5      |
| 14.394 | 19.500 | 22.053 | 5.961 | 16.092    | 28.014    | 10     |
| 18.894 | 21.000 | 22.053 | 6.072 | 15.981    | 28.125    | 5      |

TABLE V  
PRELIMINARY EXPERIMENT

Measurement of writing pressure while writing the answers to problems. An experimental period consisted of 18 consecutive problems: 6 under quiet conditions, 6 under distraction (phonograph) and 6 under quiet conditions again. Based on frequency curves of 68 experimental periods

| Mode   | Median | Mean   | M.D.  | M. - M.D. | M. + M.D. |
|--------|--------|--------|-------|-----------|-----------|
| 9.840  | 10.000 | 10.080 | 2.341 | 7.739     | 12.421    |
| 8.871  | 9.375  | 9.627  | 2.340 | 7.287     | 11.967    |
| 10.071 | 9.735  | 9.567  | 2.250 | 7.317     | 11.817    |
| 10.092 | 10.100 | 10.104 | 2.280 | 7.824     | 12.384    |
| 7.601  | 9.409  | 10.313 | 2.650 | 7.663     | 12.963    |
| 8.172  | 9.500  | 10.164 | 2.700 | 7.464     | 12.864    |
| 9.906  | 9.750  | 9.672  | 2.562 | 7.110     | 12.234    |
| 8.898  | 9.722  | 10.134 | 2.489 | 7.645     | 12.623    |
| 11.305 | 10.375 | 9.910  | 2.289 | 7.621     | 12.199    |
| 10.508 | 10.100 | 9.896  | 2.393 | 7.503     | 12.289    |
| 10.041 | 10.083 | 10.104 | 2.304 | 7.800     | 12.408    |
| 9.381  | 9.863  | 10.104 | 2.453 | 7.651     | 12.557    |
| 11.331 | 10.643 | 10.299 | 2.972 | 7.327     | 13.271    |
| 10.123 | 10.071 | 10.045 | 2.737 | 7.308     | 12.782    |
| 8.580  | 9.666  | 10.209 | 2.864 | 7.345     | 13.073    |
| 10.063 | 10.071 | 10.075 | 2.618 | 7.457     | 12.693    |
| 9.008  | 9.500  | 9.746  | 2.556 | 7.190     | 12.302    |
| 9.527  | 9.643  | 9.701  | 2.581 | 7.120     | 12.282    |

TABLE VI  
 WRITING-PRESSURE MEASUREMENTS  
 The following values are based on frequency curves of 183  
 experimental periods

| Mode  | Median | Mean   | M.D.  | M. - M.D. | M. + M.D. |
|-------|--------|--------|-------|-----------|-----------|
| 5.849 | 7.921  | 8.957  | 3.614 | 5.343     | 12.571    |
| 6.575 | 7.974  | 8.768  | 3.286 | 5.482     | 12.054    |
| 6.111 | 7.853  | 8.724  | 3.424 | 5.300     | 12.148    |
| 6.412 | 8.310  | 9.259  | 3.622 | 5.637     | 12.881    |
| 6.604 | 8.630  | 9.643  | 3.934 | 5.709     | 13.577    |
| 5.927 | 8.033  | 9.086  | 3.757 | 5.329     | 12.843    |
|       |        |        |       |           |           |
| 6.881 | 9.011  | 10.076 | 4.029 | 6.047     | 14.105    |
| 7.295 | 8.763  | 9.497  | 3.848 | 5.649     | 13.345    |
| 7.909 | 9.447  | 10.216 | 3.871 | 6.345     | 14.087    |
| 7.559 | 8.909  | 9.584  | 3.942 | 5.642     | 13.526    |
| 7.418 | 9.024  | 9.827  | 3.788 | 6.039     | 13.615    |
| 7.190 | 9.312  | 10.373 | 3.984 | 6.389     | 14.357    |
|       |        |        |       |           |           |
| 7.893 | 9.283  | 9.978  | 3.555 | 6.423     | 13.533    |
| 7.333 | 8.563  | 9.178  | 3.754 | 5.424     | 12.932    |
| 7.992 | 9.100  | 9.654  | 3.543 | 6.111     | 13.197    |
| 7.222 | 8.786  | 9.568  | 3.690 | 5.878     | 13.258    |
| 6.835 | 8.329  | 9.076  | 3.520 | 5.556     | 12.596    |
| 6.796 | 8.442  | 9.265  | 3.750 | 5.515     | 13.015    |

TABLE VII  
 WRITING-PRESSURE MEASUREMENTS  
 The following values are taken from the work of 38 Ss without  
 previous training

| Mode   | Median | Mean  | M.D.  | M. - M.D. | M. + M.D. |
|--------|--------|-------|-------|-----------|-----------|
| 5.217  | 7.125  | 8.079 | 2.472 | 5.607     | 10.551    |
| 4.816  | 6.500  | 7.342 | 2.801 | 4.541     | 10.143    |
| 6.552  | 7.500  | 7.974 | 2.079 | 5.895     | 10.053    |
| 5.948  | 7.000  | 7.526 | 2.213 | 5.313     | 9.739     |
| 5.948  | 8.000  | 9.026 | 3.561 | 5.465     | 12.587    |
| 6.268  | 7.300  | 7.816 | 2.860 | 4.956     | 10.676    |
|        |        |       |       |           |           |
| 8.078  | 8.500  | 8.711 | 2.711 | 6.000     | 11.422    |
| 8.036  | 8.100  | 8.132 | 2.514 | 5.618     | 10.646    |
| 9.458  | 9.100  | 8.921 | 2.249 | 5.883     | 10.381    |
| 9.611  | 8.625  | 8.132 | 2.388 | 5.744     | 10.520    |
| 10.552 | 8.500  | 7.474 | 2.210 | 5.264     | 9.684     |
| 8.115  | 8.091  | 9.079 | 2.091 | 5.988     | 10.170    |
|        |        |       |       |           |           |
| 7.226  | 7.900  | 8.237 | 2.643 | 5.594     | 10.880    |
| 6.658  | 7.500  | 7.921 | 2.500 | 5.421     | 10.421    |
| 7.132  | 7.500  | 7.684 | 2.368 | 5.316     | 10.052    |
| 7.422  | 8.000  | 8.289 | 2.305 | 5.984     | 10.594    |
| 7.394  | 7.500  | 7.553 | 2.132 | 5.421     | 9.685     |
| 9.410  | 8.750  | 8.420 | 2.450 | 5.790     | 10.870    |



TABLE VIII  
WRITING-PRESSURE MEASUREMENTS

The following values are taken from the work of 38 Ss one week after the first run tabulated in Table VIII

| Mode  | Median | Mean  | M.D.  | M. - M.D. | M. + M.D. |
|-------|--------|-------|-------|-----------|-----------|
| 4.423 | 7.071  | 8.395 | 3.499 | 4.896     | 11.894    |
| 5.208 | 7.666  | 8.895 | 3.446 | 5.449     | 12.341    |
| 6.763 | 7.833  | 8.368 | 3.091 | 5.277     | 11.459    |
| 5.288 | 7.166  | 8.105 | 2.402 | 5.703     | 10.507    |
| 7.328 | 8.250  | 8.711 | 3.169 | 5.542     | 11.880    |
| 6.606 | 7.500  | 7.947 | 2.789 | 5.158     | 10.736    |
|       |        |       |       |           |           |
| 7.314 | 8.666  | 9.342 | 3.537 | 6.805     | 12.879    |
| 5.342 | 7.500  | 8.579 | 2.828 | 5.751     | 11.407    |
| 7.106 | 8.000  | 8.447 | 2.997 | 5.450     | 11.444    |
| 5.447 | 7.833  | 9.026 | 3.666 | 5.360     | 12.692    |
| 7.606 | 8.500  | 8.947 | 3.474 | 5.473     | 12.421    |
| 4.468 | 7.700  | 9.316 | 3.715 | 5.601     | 13.031    |
|       |        |       |       |           |           |
| 7.322 | 8.300  | 8.789 | 3.094 | 5.695     | 11.883    |
| 6.342 | 6.900  | 7.179 | 2.660 | 4.519     | 9.839     |
| 8.611 | 9.100  | 9.263 | 3.515 | 5.748     | 12.778    |
| 4.694 | 7.214  | 8.474 | 3.388 | 5.086     | 11.862    |
| 4.258 | 6.700  | 7.921 | 2.957 | 4.964     | 10.878    |
| 4.500 | 6.500  | 9.316 | 5.256 | 4.060     | 14.572    |

TABLE IX  
TIME MEASUREMENTS

The following values are taken from the work of 38 Ss two weeks after the first run tabulated in Table III and one week after the second run tabulated in Table IV

| Mode   | Median | Mean   | M.D.  | M. - M.D. | M. + M.D. | Errors |
|--------|--------|--------|-------|-----------|-----------|--------|
| 16.626 | 18.454 | 19.368 | 4.637 | 14.731    | 24.005    | 7      |
| 15.448 | 16.500 | 17.026 | 4.738 | 12.288    | 21.764    | 13     |
| 16.965 | 18.111 | 18.684 | 3.512 | 15.172    | 22.196    | 9      |
| 12.368 | 17.000 | 19.316 | 5.277 | 14.039    | 24.593    | 7      |
| 21.842 | 21.000 | 20.579 | 4.368 | 16.211    | 24.947    | 4      |
| 13.684 | 17.000 | 18.658 | 5.296 | 13.362    | 23.954    | 8      |
|        |        |        |       |           |           |        |
| 20.448 | 20.500 | 20.526 | 5.291 | 15.235    | 25.817    | 9      |
| 16.000 | 18.000 | 19.000 | 4.737 | 14.263    | 23.737    | 12     |
| 16.684 | 19.000 | 20.158 | 5.244 | 14.914    | 25.402    | 5      |
| 17.336 | 18.200 | 18.632 | 4.067 | 14.565    | 22.699    | 9      |
| 14.842 | 18.000 | 19.579 | 4.407 | 15.172    | 23.986    | 5      |
| 14.368 | 17.000 | 18.316 | 4.715 | 13.601    | 23.031    | 11     |
|        |        |        |       |           |           |        |
| 20.684 | 21.000 | 21.158 | 5.684 | 15.474    | 26.842    | 9      |
| 17.790 | 18.000 | 18.105 | 4.280 | 13.825    | 22.385    | 5      |
| 18.052 | 19.000 | 19.474 | 5.000 | 14.474    | 24.474    | 5      |
| 15.294 | 17.800 | 19.053 | 4.798 | 14.255    | 23.851    | 11     |
| 17.106 | 19.000 | 19.947 | 4.789 | 15.158    | 24.736    | 7      |
| 17.682 | 18.666 | 19.158 | 4.737 | 14.421    | 23.895    | 5      |

TABLE X  
WRITING-PRESSURE MEASUREMENTS

The following values are taken from the work of 38 Ss two weeks after the first run tabulated in Table VII and one week after the second run tabulated in Table VIII

| Mode  | Median | Mean   | M.D.  | M. - M.D. | M. + M.D. |
|-------|--------|--------|-------|-----------|-----------|
| 6.175 | 7.725  | 8.500  | 3.765 | 4.737     | 12.263    |
| 5.184 | 7.500  | 8.658  | 3.235 | 5.423     | 11.893    |
| 4.937 | 7.277  | 8.443  | 3.149 | 5.298     | 11.596    |
| 7.735 | 8.333  | 8.632  | 4.519 | 4.113     | 13.151    |
| 5.328 | 7.250  | 8.211  | 3.285 | 4.926     | 11.496    |
| 7.657 | 8.833  | 9.421  | 3.720 | 5.701     | 13.141    |
| 6.434 | 8.2500 | 9.158  | 3.400 | 5.758     | 12.558    |
| 5.750 | 8.250  | 9.500  | 4.158 | 5.342     | 13.658    |
| 8.526 | 9.000  | 9.237  | 3.525 | 5.712     | 12.762    |
| 8.076 | 9.166  | 9.711  | 4.289 | 5.422     | 14.000    |
| 5.658 | 8.500  | 9.921  | 4.334 | 5.587     | 14.255    |
| 7.815 | 9.833  | 10.842 | 4.247 | 6.595     | 15.089    |
| 8.342 | 9.500  | 10.079 | 2.999 | 7.080     | 13.078    |
| 8.268 | 9.300  | 9.816  | 3.753 | 6.063     | 13.569    |
| 9.420 | 9.666  | 9.789  | 3.695 | 6.121     | 13.511    |
| 7.342 | 8.500  | 9.079  | 3.525 | 5.554     | 12.604    |
| 7.078 | 8.500  | 9.211  | 3.896 | 5.315     | 13.107    |
| 7.09  | 8.100  | 8.605  | 3.090 | 5.515     | 11.695    |

writing-pressure values. Table IX and Diagram E (Fig. 2) give the results of the third consecutive run for the same 38 Ss, the runs being one week apart, and Table X gives the corresponding writing-pressure measurements. Diagram F shows the reaction-times for the 20 Ss who were the most rapid in addition of the digits, and Diagram G shows the reaction-times for the 21 Ss who were the slowest in ability in the problems. In these latter two diagrams the reaction-times have been multiplied by a factor to reduce their curves approximately to the same level. Diagram H represents the author's conception of the effect of distraction.

Evidence of automatization of problem-solving does not seem to be present in a single sitting. There is no increase in the speed of working in the first 6 problems of the 'normal' period; and there is no appearance of a lowering of the level of the means in the entire run anywhere. But when we examine Tables III, IV and IX and Diagrams C, D, and E (Figs. 1, 2) we note that in the successive runs for the same Ss there is a distinct increase in the speed of working the problems, accompanied by a distinct decrease in the number of errors. It appears that the lapse of a week between sittings seems to bring out a tendency toward auto-

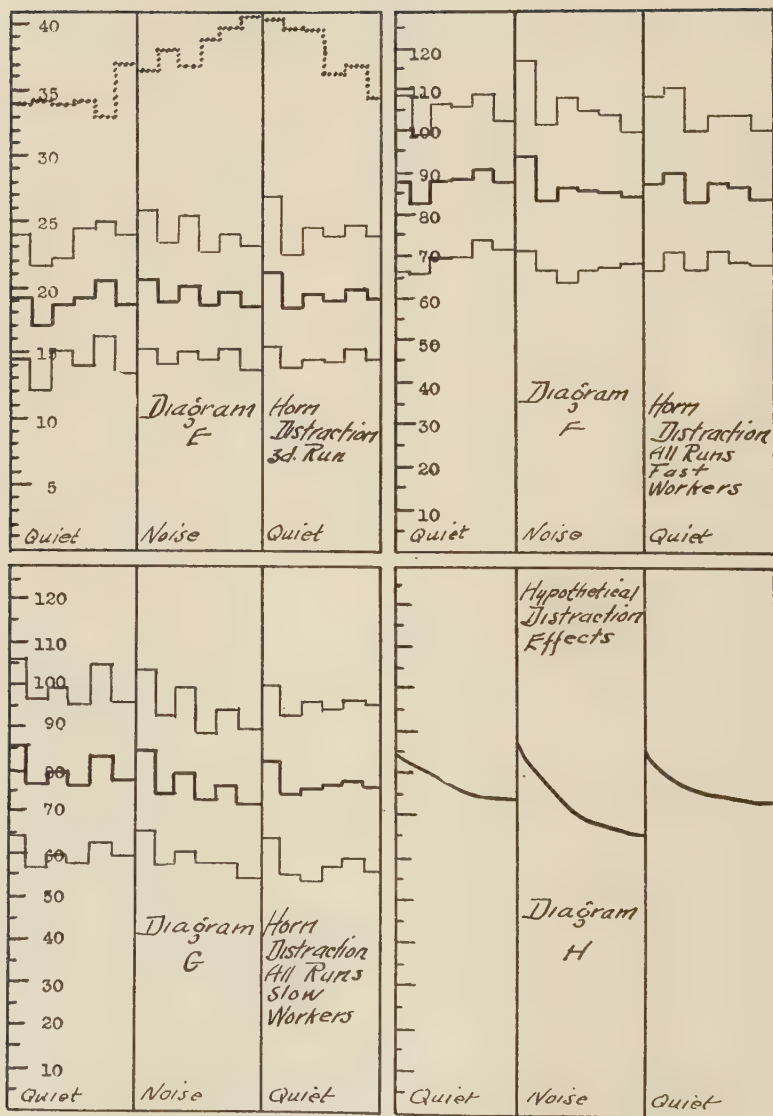


FIG. 2



matization not observable in the course of one sitting. The first problems of the second sitting were always done more rapidly than the last problems of the previous sitting. If automatization is present in the first sitting it is obscured by the presence of fatigue or other unknown factors. The third consecutive sitting shows an initial reaction-time still shorter than the last problems of the second sitting. The difference between the first and second, however, is greater than the difference between the second and third, evidence that automatization is a process of gradual approach to a limiting level.

The most conspicuous point in the examination of the graphs of the tabular evidence is the initial slowing in reaction-time at the beginning of the noise-period and the beginning of the following quiet-period. The mode is thrown toward the center of the frequency curve in all cases and evidence indicates that the effect of the noise is on all reactions, not on a select few. This means that *both the noise and the quiet have constituted distractions*. But the initial slowing is more pronounced at the beginning of the noise-period than at the beginning of the following quiet-period. In all probability this is explained by the difference in the attitudes of the Ss at the two different times. The automobile horn has certain meanings that produce stronger emotions than are found at the beginning of the last quiet-period. Apparently it is not the absolute presence of the noise that constitutes a distraction, but the fact that we have a change in the pattern-stimulus which changes the integration determining the reaction.

Introspective evidence came in great abundance. "When the quiet came on I missed the noise and wished it had not stopped." "When the horn stopped I was astonished because I had forgotten it was going," came in substance from 5 Ss, although the horn was so loud that no other sound could have been heard. Other Ss said that they "missed the racket," that they were bothered by the "uproarious quiet," that they could "hear the silence" and knew they were not doing as well as they expected. Often Ss would report that they wished the noise would stop and when it did stop they wished it had not. Occasionally an S reported having liked the noise because it produced an exciting effect. Most of the Ss were certain that they worked more slowly during the noise-period, and were quite sure they worked somewhat faster when the noise stopped, a conclusion which was somewhat divergent from fact.

Viewed from a somewhat different angle, the above results place an entirely new light on our method of defining a distraction. It means that the effectiveness of one pattern to disrupt another

is not at all a matter of absolute intensity but rather the presence of a stimulus gradient in a given unit of time. Whether the gradient is negative or positive, that is, descending or ascending, the mental effects on integration are much the same.

A summary of these curves would show the following points.

(1) The mean gets lower with each lapse of a week, but does not drop so much between the second and third sittings as between the first and second.

(2) The mean deviation gets constantly smaller and smaller. This may be considered the result from the dropping out of uncontrolled distracting influences and attitudes.

(3) Skewness diminishes during the distraction, an indication that the distracting effect is spread over the short reactions.

(4) A pronounced retardation is found at the beginning of the quiet-period after the noise-period. Both noise and quiet are distractive.

(5) Distraction seems to have very little effect on errors. Probably the *Ss* paid more attention to accuracy than to time. There are always more errors in the first half of a period, however, than in the second half.

(6) The effect of the distraction is about equal for rapid workers and for slow workers in the first problem of the noise and quiet. But the rapid workers make a very prompt recovery from the effects of the distractions, while the slow workers require nearly the entire 6 problems before the recovery is complete. Apparently the slow workers are unable to build up automatized resistance against the distraction.

## VII. WRITING-PRESSURE AND BREATHING

Morgan seems to have believed that the strength of the motor processes was in some way an index to the intensity of the mental events as differentiated from speed. There is reason to believe, in our experiment, that muscular tonus, once built up at the beginning of a distraction, tends to stay. Had we continued our sittings each over as long a period as did Morgan, there is a probability that we should have secured the same results. The general contention of Morgan, however, that the increase of speed of his *Ss* during noise-intervals was accomplished through the expenditure of an unusual amount of motor energy does not seem to harmonize with our writing-pressure curve.

Noting the graphs in Diagrams C, D, and E, it is quite evident that there is almost no correlation at all between the mean of the writing-pressure curve (the dotted line) and the mean of reaction-times. These are based on only 38 runs, however, and the samples are not large enough to show results free from chance variability. But if we combine all the sittings of the entire experiment, as they are illustrated in Diagram B, we have the rather surprising illustration of the statistical fact that when we add together several non-correlated pairs of measurements, as in Tables III, IV, and IX, and the measurements of 3 *Ss* not included in these tables, but whose data also showed no correlation, we may get a very reliable correlation between writing-pressure and reaction-time in the larger table. Using the formula

$$r = \frac{\Sigma xy}{\sqrt{\Sigma x^2 \cdot \Sigma y^2}},$$

we find that the correlation between the values expressed in Tables II and VI is 0.55. The cropping out of this correlation from the addition of the three smaller tables together, and the evidence not tabulated, can be explained by the fact that with enough cases the negative and positive deviations from the mean tend to cancel each other and leave values which are the real and universal effects of the distraction.

Since we have here a positive correlation between writing-pressure and reaction-time, and since we are assuming that the presence of long reaction-time is an indication of lack of automatization and of the presence of attention, it would seem that in the end we must abandon any theory that continuous and rapid work under noise conditions will be accomplished at a sacrifice of muscular energy. In all probability the functioning of motor tonus in some way or other facilitates the building of new integrations, and when these integrations are well enough established to work without interference from other pattern-stimuli, the reinforcement of the proprioceptive excitations from motor tonus is no longer needed.

The evidences of articulation in the breathing curves for our experiment resemble the descriptions given by Morgan and in all probability were as nearly identical as possible. We do not believe, however, that it is possible to produce as exact a ratio between inspiration and expiration as he seemed to give. Due to the fact that there were inhibitions of breathing frequently occurring, it was very difficult to decide whether a given portion of the curve was to be regarded as an inspiration or an expiration.



## VIII. VASO-MOTOR PHENOMENON

While the various theories for the relation of the vaso-motor phenomenon to the fluctuation of attention did not concern us here to any great degree, we had thought at the beginning of the experiment that we might investigate the possibility of a connection between the strength of the phenomenon and the degree of attention, if our facts gave us a warrant for using the phrase "degree of attention."

A review of the early theories of the relation of blood circulation to attention has been given in Pillsbury's<sup>20</sup> work on attention. The studies and theories show the usual appearance of early work on new and complex organic principles. There seems to be a general tendency to regard the Traube-Hering wave as a simple phenomenon rather than complex. This tendency to make mental and physical processes too simple has permeated the entire history of man's investigations in the realm of biology. If the changes in mental integration we choose to call attention have anything to do with the changes of the Traube-Hering wave, it is certain that their part may be obscured by other strong forces of non-mental origin.

The early theories that the peripheral constriction of the capillaries and arteries is accompanied by a cerebral passive dilatation must be held with considerable doubt in the face of the more recent evidence that there may be active constriction in the cerebrum. Huber<sup>21</sup> found evidences of efferent fibres terminating in the cerebral capillaries. Wiggers<sup>22</sup> showed the possibility of active constriction in the cerebrum. Shepard<sup>23</sup> found evidences of correlation between vaso-constriction of the cranium, in trephined cases, and vaso-constriction of the periphery. We do not wish to treat of this controversy in detail, but merely to indicate that at the present time any anaemia theory of the relation of blood circulation to attention is certain to find strong opposition in some scientific quarters. We may not assume here with any great degree of safety that the adjustment of blood circulation is the cause of attention.

But there seems to be a considerable amount of evidence that the objectively controlled changes in attention may bring about a change in blood circulation. Angell and Thompson<sup>24</sup> reported certain characteristic shapes of the vaso-motor curve with mental events. If attention were to be considered as a state, unvarying and constant, and if there were a constant correlation between the amount of vaso-constriction and the degree of attention, we should expect to find the degree of constriction tending to take on a certain level and keeping that

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<sup>20</sup>W. B. Pillsbury, *Attention*, 1908, 75-80.

<sup>21</sup>G. C. Huber, Observations on the innervation of the intracranial vessels, *J. Comp. Neur.*, 9, 1899, 1-25.

<sup>22</sup>Wiggers, Action of adrenalin on the cerebral vessels, *Amer. J. Physiol.*, 14, 1905, 452-465.

<sup>23</sup>J. F. Shepard, *Circulation and Sleep*, 1914.

<sup>24</sup>J. R. Angell and H. B. Thompson, A study of the relations between certain organic processes and consciousness, *Psychol. Rev.*, 6, 1899, 32-69.

level as long as the attention remained constant. But anyone who has ever worked with the vaso-motor phenomenon knows that it is comparatively easy to produce a prompt constriction (the total change occurring often in as short a duration as three seconds) by all sorts of stimuli, but that it is very difficult to secure an equally prompt vaso-dilatation. The process seems to work rapidly only one way; the dilatation is usually slow when it is of the *active* (nerve-controlled) type. Passive dilatation may be secured just as rapidly as active constriction. We may note from Angell and Thompson's work that the most characteristic process is a rapid initial constriction followed by a slow dilatation for any and all stimuli except possibly the mental arithmetic, from which they did not seem to secure curves with uncontroversial evidence. But if the term attention cannot refer to a constant condition, but always becomes complicated with gradual automatization, then it is very possible that the degree of peripheral vaso-constriction is partially correlated with the degree of attention, assuming that the greatest degree of attention is secured only at the beginning of a work-period.

The invention of the Lombard-Pillsbury plethysmograph gave the laboratory an instrument which is very delicate in recording the capillary vaso-motor changes uncomplicated with the changes of the larger arteries; although it is perfectly true that the instrument is very difficult for quantitative calibration. Our use of this instrument indicates that the same type of curves holds for initial attention as were secured by Angell and Thompson for the more violent affective and emotional stimuli. We are exceedingly skeptical concerning the possibility of any attention experiment whatever in which feeling and emotional elements are well controlled, and are inclined to believe that the conditions for both affection and attention are identical.

During the fall and winter of 1919 we had occasion to accumulate 411 records of the vaso-motor phenomenon taken with the task of giving the fluctuating responses from stimuli of ambiguous perspective and binocular rivalry. About a fifth of these records were not of much value because of the fact that some of the *Ss* displayed a continuous tendency to a strong constriction due to cold hands.

There were 91 *Ss* in all, 26 women and 65 men. An *S* was placed on one side of a partition, faced either with a card on which was drawn the ambiguous figure or with the stereoscope in which there was placed a card with a pair of oppositely colored squares which are ordinarily used to produce retinal rivalry. The Lombard-Pillsbury plethysmograph was fastened to the forefinger of *S*'s left hand. The right hand moved a lever laterally to and fro to indicate the changes in fluctuation, which were all indicated on the smoked-paper kymograph drum. *S* was given a normal run, then a period of from 1 to 2 min. of fluctuation responses, then a rest-period of about 2 min., then another run giving the fluctuation responses. At the beginning of the experiment *S* began with the word 'start' and stopped at the word 'rest.' Sometimes there was a

terminal rest-period and sometimes there was not. We did not tabulate the vaso-motor phenomenon in the initial 'normal' period, and so we have 427 'work' periods tabulated as against 339 'rest' periods.

The following points were observed and statements of probability devised for each. (1) A rapid initial vaso-constriction followed by a slow dilatation. This meant that the constriction in the first 10 sec. must be more pronounced than at any other portion of the period. (2) A rapid initial vaso-dilatation followed by a slow constriction, or maintaining its level. (3) A level curve in which initial changes were either absent or so weak as to be unobservable. (4) An irregular curve in which the variations were deep but so constant that initial effects could not be observed. (5) Permanent constriction due to cold hands. (6) Vaso-constriction initially set up and maintained at a constant level throughout the period.

The situations above enumerated were as follows:

#### WORK-PERIODS

(427 sittings)

(1) 51% (2) 0.23 of 1% (3) 21.8% (4) 7% (5) 19.97% (6) 0%

#### REST-PERIODS

(339 sittings)

(1) 50.1% (2) 0.29 of 1% (3) 21.3% (4) 11.5% (5) 16.81% (6) 0%

The interpretation of the above shows that there is but one set of figures which warrants a positive statement; the tendency with a homogeneous series of acts for initial vaso-constriction followed by gradual dilatation. The statement is true of both rest- and work-periods.

Vaso-motor records from the distraction experiment using the motor horn as a distractor were more productive of results because of the greater reliability of the plethysmograph used. In 18% of the distraction records there was a one-to-one correspondence between problems and vaso-constriction from start to finish. In 66% of the records there were fragmentary periods of one-to-one correspondence, varying from 4 to 10 successive correspondences. The remaining records seem to show no definite correlation between the vaso-motor fluctuation and the separate problems.

In 183 distraction records there were 161 noise-periods in which there was an initial vaso-constriction stronger than in any other portion of the record. There were 94 of these records showing initial constriction in the last quiet period stronger than in any other place in that period. The measurement of vaso-motor effects during the distraction experiments merely verified the shape of the curve already attained from other evidence—that any *change* of mental processes, whether we call them attention, emotion, feeling, or what not, produces an initial constriction followed by gradual dilatation. Certainly at the beginning of the noise-period emotions were present, but this made no difference in the relative shape of the vaso-motor curve.



Although it certainly seems true that there were so many complex factors lending their forces to change the shape of the vaso-motor curve that we must be very cautious in using the Traube-Hering wave as an index to anything, it does seem that such interrelations as we have found favor a curve gradient as a concept of the nature of attention.

#### IX. SUMMARY

Efficiency of work performed evidently goes through a definite transition as time elapses with environmental stimuli constant. Since names should not commit one to the assumption of faculties or other mental entities, we may use some name for the nature of that mental coördination which exists at the beginning of a new and novel stimulus combination, and because this coördination seems to have certain signs which resemble the conditions described by past workers in the field of psychology as *attention* we feel justified in whatever violence we may perpetrate in using the same name for the beginning of a synthesis of reactions to a novel environmental pattern. But because the following reactions seem to display definite differences in their capacity to show efficiency in the face of foreign distracting stimuli, we wish to give another name to the behavior which follows, and again we may borrow from the field of psychology that term which suggests past evidence resembling as closely as possible the evidence in our own work. The term *habituation* has been used, but without a clear enough distinction from the word *attention*. The word *automatization* has been used in the field of abnormal psychology as indicative of behavior without consciousness or behavior without attention. Again we may not quibble concerning the justification of the use of the term *consciousness*. The behavior in the later parts of the distraction-period and the behavior of a person exhibiting automatisms shows a similarity of form and conditioning. Possibly we may say that it tends to become unconscious, and therefore unattended. From a purely behavioristic point of view we have the certain distinction that initial reactions are highly susceptible to interference from new and heretofore unexperienced stimuli. Conversely, later behavior of the same type is capable of a certain capacity to resist interference from sudden and novel stimuli. The transition from the one to the other may be called the psychodynamic gradient.

The evidence for such a concept of attention-automatization may be listed as follows.

(1) In every case the initial reaction is always longer than any other reaction of the period having constant environmental stimulus patterns. This is true of both quiet and noise periods and in excess of any training effect.

(2) There is a slight tendency for more errors to be committed in the first half of a given homogeneous period than in the second half; although there were not enough cases of error in our evidence to warrant problem-to-problem comparison.

(3) The writing-pressure and reaction-time showed a reliable positive correlation of 0.55, indicating that new integrations of behavior were accompanied by evidences of general motor tonus, and, as time passes, this tonus likewise exhibits signs of a gradient. Motor tonus becomes somewhat of an index to the degree of attention, *as we have defined attention*. At this point it is well to warn the reader that this statement of the fluctuation of motor tonus is not entirely in agreement with Morgan's statement in that he apparently holds that the tonus continues through the distraction-period and that the general average of high efficiency for that period is due, not to automatization, but to some intense mental process which he does not name.

(4) Vaso-motor concomitants likewise show initial effects which go through gradient changes as the same type of work continues under constant environmental stimuli.

(5) Fast workers, probably individuals who, either from natural aptitude or from past experience, very rapidly acquire automatization of behavior in this work, show almost an instantaneous recovery after the first problem under the noise. Slow workers acquire a facility only gradually, a fact which appears reasonable in view of the possibility that the slowness may be due to inadequate past experience or the fact that, in acts of skill, speed is the accompaniment of automatization. The recovery from the initial effect of the distraction did not fully occur until the last problem of the series. Shortness of reaction-time apparently becomes a measurement of automatization whether we are comparing different records of the same individuals or composites of records from different groups. The curve-gradient is the most conspicuous element in every picture of temporal progress.







